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APPLIED BEHAVIOR ANALYSIS AS TECHNOLOGICAL SCIENCE

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Since its inception, the *Journal of Applied Behavior Analysis* (*JABA*) has emphasized the publication of research involving applications of the experimental analysis of behavior to problems of social importance. These features of *JABA* and the larger field that it represents were clearly described in the seminal article by Baer, Wolf, and Risley (1968) and have been reaffirmed numerous times subsequently (e.g., Azrin, 1977; Baer, 1978). Thus, there always has been a close link between basic and applied research in behavior analysis, even though that link may not be formally stated in every article published.

In examining the generality of behavioral principles with socially important responses, applied research has produced a methodology and technology of behavior change analogous to those found in other scientific endeavors having social impact. All fields of science that have produced methods for quantification of data and control over their subject matter also have shared a research orientation heavily emphasizing the development of technology and its translation into effective behavior (e.g., of the architectural, surgical, and airplane manufacturing sort). To the extent that applied behavior analysis represents a scientific *and* practical approach to the study of behavior, its technological character is essential.

But is our emphasis on technology excessive? Hayes, Rincover, and Solnick (1980) answered the question affirmatively. Independent of the data on which their conclusion was based (only one of 15 sets of data supported that conclusion[1]), it would be difficult to argue that technology in *any* field of science is excessive because the only direct consequence of improved technology is increased precision. Better precision leads to better experimental control that, in the case of applied behavior analysis, yields benefits in both application and extension of basic science. Our ability to both analyze behavior and develop consistently effective behavior-change procedures is entirely dependent upon further improvements in technology.

The critique of applied behavior analysis did not end with technology, however. Hayes et al. (1980) proposed that applied behavior analysis is not only "technological to a fault" but also "atheoretical to a fault." This criticism is more difficult to refute because it requires clarification of several issues: What is theoretically relevant research? Does applied behavior analysis research emphasize technology over theory? What is the role of theory in the further development of our field?

What Is Theoretically Relevant Research?

Hayes et al. (1980) defined theoretical research as that showing

> an effort to advance our basic knowledge of some behavioral phenomenon. In addition to "how to" questions, this type of research also asks "what is" questions, such as "what is the nature of imitation in children" or "what is a response class?" (p. 278)

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[1] The relevant data set was a small proportion of *JABA* studies in which procedural descriptions were not labeled with respect to their underlying basic principles. Hayes et al. defined these studies as "purely technical articles," in the sense that there was nothing theoretically relevant about the studies. Yet the definition has at least two limitations. First, "technical" was not described with respect to any inclusion criteria (i.e., what must a study contain in order to be considered technical?). Instead, the authors defined technical by exclusion; omitting reference to a basic principle is not the same as describing procedures in a clear and operational (technical) manner. Second, it seems that the authors used a structural or topographical definition for technical rather than a functional one; they later reprobated this very approach when discussing how analogue studies can be considered relevant to application if one uses a functional rather than procedural description for "applied."

Using this definition, it is difficult to determine "what is" or is not an instance of theoretical research. For example, consider the extent to which several types of research questions about imitation are "theoretical" in nature.

1. Is imitation an operant response? The answer is provided simply by arranging a contingency between imitation and a suitable consequence such as praise.

2. Is imitation sensitive to intermittent reinforcement? The answer is provided by arranging the contingency for a proportion of imitative responses.

3. Does reinforcement of imitative behavior produce generalization (e.g., to nonreinforced imitative responses)? The answer is provided by reinforcing some imitative topographies but not others, while taking data on both.

4. What techniques can be used to improve imitation in developmentally handicapped children? The answer is provided by performing any of the above operations with a particular individual serving as subject.

5. What can be done when procedures used to answer Question 4 sometimes are associated with behavior other than imitation (i.e., disruption)? The answer is provided by varying the consequences for imitation and/or disruption; alternatively, one can begin asking a series of questions about disruption per se (Is it an operant?, etc.).

Each of the five questions asks about the "nature" of imitation ("what is?") and related behaviors in a way that can only be answered through technological arrangement of the environment ("how to?"). Moreover, one could argue that none of the answers produces a new theoretical concept but that all extend operant theory. The first question is interesting and likely to be asked when little is known about imitation; the last is interesting when more is known about imitation. This seems like a reasonable developmental progression of research that can easily account for a predominance of the last question, and variations thereof, after the first question has been answered a few times. The only differences across questions are how the contingencies are arranged, how the data are collected, and how the subjects are selected. More effort, control, and technology are required as one moves from the first to the last question. Yet the first question might be regarded as theoretical but the last as purely technological, and the first question will remain theoretical even after it has been answered frequently enough to become uninteresting. It is not clear how asking less and doing less (technology) translate into more (theory).

If imitation as an example is too simple because it has been the subject of behavioral research for over 20 years, more recent topics can be substituted. In behavior analysis research with developmentally handicapped individuals, two areas of current interest are the establishment of conditional discriminations (i.e., stimulus equivalence classes) in response acquisition and functional analysis approaches to response reduction. Both are regarded as major theoretical as well as technological areas of research. Yet neither has produced new theory. Instead, previously identified functional relations—described as stimulus control and response maintenance—have been extended through technological refinement, and already we are seeing very rapid progression from "what is" to "how to" questions. Given the potential interchangeability between these types of questions, an alternative to the claim that "some research is less theoretical" can be proposed: "All behavioral research is theoretically relevant, but some research is more applied." Neither describes an ideal taxonomy, but the limited utility of the first statement may become apparent to some only when it is placed in contrast to the second.

Does Applied Behavior Analysis Research Emphasize Technology over Theory?

Another way to consider the issue of technological versus theoretical research is to ask whether there is too little theory. This question cannot be answered definitively because it assumes that the consequences of too much or too little are known. Nevertheless, when viewed in relationship to other fields of science, the ratio of technology to theory in behavior analysis is probably no larger than that seen in physics or biology, and perhaps considerably smaller. It is only when behavior analysis is viewed

in relation to the rest of psychology that our emphasis on technology may seem excessive or our use of theory limited. The other major psychological systems—for example, psychoanalysis, which dominated the study of human behavior early in the century, and cognitivism, which has taken its place—have not produced a practical science of behavior precisely because their subject matter remains elusive. These fields are left with theory as an explanation of behavior in the absence of confirming data. By contrast, behavior analysts have shown repeatedly that it is possible to exert reliable control over behavior by systematically varying its consequences and events correlated with them. So, our "theories" (e.g., about reinforcement contingencies, schedules, stimulus control, etc.) are different from most psychological theories because they are not speculations about how uncontrolled phenomena *might be* controlled. Most of our theories exist as functional relations describing how phenomena *have been* controlled.

At the present time, there is still much about environment–behavior interactions that we do not know (i.e., that we have not been able to control), and all of what we do know has yet to be translated into effective application. Therefore, a small proportion of our field consists of "might be" theories, but these are often short-lived as vacant predictions about behavior because they are quickly replaced by technological data. Thus, our theories, like those in the physical sciences, are both derived from and extended by technological demonstrations of experimental control. In much of psychology, exciting theories—guesses about behavior—are offered instead of a technology of behavior based on experimental control. And psychoanalysis is dead not because of a lack of theory or theoretical research but because of a lack of useful technology.

What Is the Role of Theory in the Further Development of Our Field?

Hayes et al. (1980) suggested that "Applied behavior analysts often seem to generate their interventions by intuition, trial and error, informal observations, or common sense, rather than theoretical concepts and analysis. . . . Such an approach . . . might actually slow progress of the field" (p. 284). But has such research *really* slowed progress? Must applied research be theory driven in order to be useful? Moxley (1989) noted that scientific theory and technology are not necessarily related in a hierarchical manner (i.e., effective technology development need not follow from or rely on existing theory). Instead, there appears to be a symmetrical relationship: Technology can be derived from either theory or technology itself and vice versa. Skinner (1950) commented more specifically on the necessity of theory to research many years ago:

> It is argued that research would be aimless and disorganized without a theory to guide it. The view is supported by psychological texts which take their cue from the logicians rather than empirical science and describe thinking as necessarily involving stages of hypothesis, deduction, experimental test, and confirmation. But this is not the way most scientists actually work. It is possible to design significant experiments for other reasons, and the possibility to be examined is that such research will lead more directly to the kind of information which a science usually accumulates. (p. 194)

Skinner's view does not discount theory as a useful controlling variable for research behavior, but it suggests that theory is not the only legitimate controlling variable. Returning to the earlier example of imitation, it can be seen that different research questions suggest different controlling variables. The first question (reinforcement control) asks how imitation develops. The second and third questions (schedule control, generalization) ask what can be done with already developed imitation. Answers to all three questions are generally informative, but also are of limited immediate benefit to therapists working with nonimitative clients or to the clients themselves. The fourth and fifth questions (establishing imitation in handicapped individuals, and doing so while reducing competing behavior) ask about imitation in specific applied contexts; the answers are immediately beneficial to therapists and clients, but are such answers generally

informative? I think so, because answers to the latter questions suggest how behaviors other than imitation might be examined in the same individuals, how imitation might be developed in other individuals, and how reinforcement procedures might be varied to examine other behaviors in other individuals.

There are, of course, additional controlling variables to consider, such as those promoting widespread adoption of technological innovation (Stolz, 1981). The point is that a multiplicity of controlling variables has advanced our field by promoting diversity. And the practical value of these diverse research efforts, not just for consumers but also for other researchers, has been a direct function of well-controlled experimentation (i.e., technology).

The Real Problem

The most serious problem evident in applied behavior analysis today is not the type of research being conducted; it is that not enough good research—of all types—is being conducted. We need studies that do nothing more than operationally define hypothetical constructs considered important in our society but not previously examined behaviorally. An example might be "healthy self-concept" in children. Once defined, we need studies that identify the environmental determinants of a healthy self-concept, the conditions under which self-concept fails to "emerge," and problems associated with "poor" self-concept. We need other studies developing methods for improving self-concept, increasing the effectiveness and efficiency of those methods, and examining the generality of those methods across subjects and settings. We need studies to identify the indirect or long-term benefits of intervention. Finally, we need to consider how our resulting technology for enhancing self-concept can be disseminated effectively.

Each of these types of research is theoretically relevant, heavily dependent on technology, and important to the further development of our field. There will always be questions about relative proportion: Some will call for more "real-world" application, whereas others call for more studies that "extend basic principles." These calls are relevant as attempts to increase the frequency of underrepresented types of research. In fact, most of us agree that more of both types of research is needed and feel that the goal can be achieved through careful prompting, modeling, and reinforcement. To reduce the frequency of one type of research in order to increase the other, however, seems imprudent. To reduce the frequency of one type of research by denigrating it or by punishing those who do it well seems foolish.

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